The End of Economic History?

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The title that I chose for this survey of recent developments in economic history, "The End of Economic History?", is one that I have regretted many times. It is one of those choices that sounded so clever late at night when I mailed it off, and in the harsh light of day, sounded somewhere between ridiculous and mean-spirited. There is a logic, however, to the title. It is a not very subtle play on the title of Francis Fukuyama's (1989) article, "The End of History?", which discusses the state of international relations and the prospects for world peace. Fukuyama argues that the cold war is over and the good guys won. The whole world now agrees that democracy is the right form of government and capitalism is the right form of economic organization; all that remains to be done is to put these institutions into place everywhere.

My view of recent developments in economic history is that the war is over and the good guys won. More concretely, the field of economic history is no longer a separate, and perhaps marginal, subfield of economics, but rather, is an integral part of the entire discipline. In this regard, economic history has come full circle from where it was, say, a century ago. Economic history was once just a part of economics: economists tried to understand what happened in the past and used the past to understand the present. Then, in the 1950s, 60s, and 70s, the "new economic history" struggled to show that economic history was just like any other subfield of economics. It used economic theory and econometric techniques to answer specific and well-posed questions about slavery, economic

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growth, and a host of other historical topics, in the same way that a labor economist might use these tools to answer questions about the effect of unions or the determinants of wages. Although this was an important step toward making the study of history as rigorous and sophisticated as the study of any other economic problem, it seemed to isolate economic history somewhat. The field became the purview of people who called themselves economic historians and published in economic history journals.

The most exciting recent development in economic history in the last decade or so is that the rest of the profession has started to join in the study. Perhaps because the new economic historians showed that historical topics could be analyzed with the same tools other economists use, researchers who might think of themselves as macroeconomists or international economists have begun to focus on historical topics. At the same time, the work of economic historians has begun to find a wider audience among specialists in other subfields of economics. The result is that economic history has come back to being a part of all of economics, rather than just a separate piece.

In this article, I describe some of the accomplishments of this melding of economic history with the rest of economics. I discuss just a small piece of the excellent research done by economic historians, and those who would call themselves probably anything but economic historians, because in a short article one cannot hope to cover much of the fine work that has been done in the last decade. I focus mainly on developments in American economic history, which is my specialty. I hope to show that in recent years our understanding of a wide range of historical topics has advanced immeasurably and that those advances have fed back to the rest of economics.

THE GREAT DEPRESSION

No area illustrates my theme better than research on the Great Depression. In the last decade, the 1930s have been a topic of extensive study by both economic historians and macroeconomists. This fruitful collaboration has not only advanced our understanding of this puzzling and frightening event, but has also served as a spawning ground for much recent work in macroeconomic theory.

Two excellent books, Temin (1989) and Eichengreen (1992), have resurrected the gold standard as an important part of the story of the Great Depression. Temin points out a crucial asymmetry in the gold standard: a country is free to run a more restrictive policy than the rest of the world because this will cause gold to flow in; but a more expansionary policy than one's neighbors will cause gold outflows and, depending on the initial size of a country's gold reserves, a possible confrontation with gold reserve limitations. Because of this asymmetry, Temin shows that deflationary shocks were passed from the United States to the rest of the world by the system of fixed exchange rates that was in place in the 1920s. For example, when the Federal Reserve decided to increase interest rates in the United States in the late 1920s in an attempt to stem stock market speculation, the rest of the world either had to go along with the restrictive policy or devalue to prevent massive gold outflows. Eichengreen (1992) and Eichengreen and Sachs

(1985) point out that as long as countries were determined to remain on the gold standard, they were largely helpless to unilaterally confront the worldwide decline in output in the early 1930s with expansionary monetary policy.

Against this institutional framework, various authors have analyzed the possible shocks that pushed the United States into decline and were then transmitted to the rest of the world through the gold standard. Hamilton (1987) shows that tight monetary policy was an important source of the onset of the recession that started in the United States in the late summer of 1929. He points out that the growth rate of M1 and M2 in 1927 and 1928 was much lower than it had been on average in the five years before. This slowdown in the growth rate of the money supply was a deliberate policy on the part of the Federal Reserve, which hoped to curb speculation in the U.S. stock market. Hamilton finds that the consequence of the tight monetary policy was that both nominal and real interest rates in the United States rose in the late 1920s, and this appears to have reduced the kind of spending that usually responds to high interest rates.

Temin (1976) shows that, although monetary policy may have been important in cooling off the hot U.S. economy of the late 1920s, the tremendous acceleration of the decline in real output that occurred at the end of 1929 and in 1930 almost surely had nonmonetary sources. Temin argues this with elegant simplicity using the IS-LM model that is taught in intermediate macroeconomics. He points out that if monetary forces were crucial, that is, if there was a large shift back in the LM curve, interest rates should have risen. In fact, however, they fell quite sharply at the end of 1929 and remained low until the financial panics began in late 1930.

Macroeconomists sometimes look at Temin's story with a critical eye because it fails to draw a distinction between real and nominal interest rates. For fixed expectations of future inflation or deflation, a negative monetary shock will clearly raise nominal interest rates. However, if the monetary policy affects expectations of future price movements, this will also shift the IS curve, which depends on the real interest rate. In the case of the Great Depression, it might be argued that tight monetary policy caused expectations of deflation, which caused real interest rates to rise and thus shifted back the IS curve. Thus, nominal interest rates could fall, but the shock, in some fundamental sense, would, nevertheless, be monetary in origin.

This understanding of the role that price expectations might have played in the Great Depression has generated several studies of the behavior of expectations and real interest rates in the 1930s. In a 1992 issue of the *American Economic Review,* Stephen Cecchetti and James Hamilton each have articles on price expectations. Cecchetti uses a technique pioneered by Frederic Mishkin to estimate real interest rates at the start of the Great Depression. Under the assumption of rational expectations, if one regresses the ex post real interest rate on lagged information about variables such as prices, output, and nominal interest rates, the fitted values of this regression provide an estimate of the ex ante real interest rate. Cecchetti finds, in Temin's defense, that the monetary contraction of the late 1920s generated expectations of only modest deflation in late 1929 and early 1930. Thus, Temin's conclusion that something other than tight monetary policy must have been depressing the U.S. economy in 1929 and 1930 seems to hold up.

Hamilton, using a somewhat different approach to analyze price expectations, argues that traded futures prices for commodities such as corn, oats, and rye may provide a direct way of assessing price expectations in the early 1930s. Although Hamilton's evidence agrees with Cecchetti that in the first year of the Great Depression there was not much expectation of deflation, the two studies disagree about the course of expectations during 1931 and 1932, the most extreme years of the Great Depression. Cecchetti's regression estimates indicate that the huge deflation of this period was expected; Hamilton's futures-prices measure indicates that much of this deflation was unanticipated. The reconciliation of these two points of view is clearly an important topic for future research.

Because both studies of price expectations confirm Temin's view that the onset of the Great Depression had nonmonetary origins, much research has been devoted to identifying just what these nonmonetary factors might have been. One approach seeks to link the stock market crash of 1929 with the rapid decline in output in late 1929 and early 1930. In the *Quarterly Journal of Economics* (1990), I argue that the extreme volatility of stock prices in the fall of 1929 and throughout much of 1930 made consumers and producers very uncertain about the course of future income. This uncertainty, I suggest, explains why consumers stopped buying irreversible durable goods and firms stopped investing in plant and equipment right after the stock market crash. To bolster this theory, I look at contemporary forecasts made by five forecasting services around the time of the Great Crash. I find more dispersion than usual across the five forecasts and the forecasters seemed less sure of their forecasts than in other years of the 1920s. Thus, the crash appears to have made professional forecasters more uncertain about the future. I then look at the usual relationship between stock market volatility and consumer spending on durable goods during the four decades before the Great Crash. I find that stock market volatility has a significantly negative effect on consumer spending on durables. Furthermore, this estimated effect is large enough that the tremendous rise in stock market volatility in 1929 can explain all of the peculiar drop in consumption that occurred in 1930.

In this article, I can see clearly the benefits of the merging of economic history and the rest of economics that is my theme. Much of my thinking about the effect of the stock market crash on real spending was influenced by a theoretical paper by Bernanke (1983a) on the impact of uncertainty on irreversible investment expenditures. In response to my article, macroeconomists have begun to examine whether stock market volatility or other measures of uncertainty should be included in modern forecasting equations for consumption. In this case, as I am sure is true in a multitude of other cases, historical research was both influenced by modern research and had an influence on work about recent conditions.

Although I think that the stock market crash can explain why the recession that started in the United States in the summer of 1929 took a very nasty turn at the end of 1929 and during 1930, it cannot explain why the economy got progressively worse during 1931 and 1932. The best explanation of the continuing decline is the financial panics emphasized by Friedman and Schwartz (1963). They show that the four waves of bank failures between the fall of 1930 and the winter of 1933 caused enormous drops in the money supply. These drops are surely part of the explanation for why real output fell so disastrously in 1931 and 1932.

However, one of the most important developments in economic history in the last decade is Bernanke's (1983b) argument that the financial panics also affected the economy in ways unrelated to the supply of money. Bernanke argues that when banks failed in the 1930s, all of the specific knowledge that those banks possessed about the credit-worthiness of local borrowers was lost. As a result, it became more expensive for the remaining banks to make loans to the small businesses that could only get credit through banks. This rise in what Bernanke terms the cost of credit intermediation either raised the cost of borrowing to small businesses or made it more difficult for such firms to get loans. In either case, Bernanke suggests that investment spending by small firms probably fell following the waves of bank failures. Bernanke backs up this insight about the effects of financial panics by simple regressions of industrial production on the unanticipated changes in the money supply and on the assets in failed banks. He finds that movements in the money supply only explain about half of the movement in real output during the 1920s and 1930s. Bank failures contribute substantially to the explanatory power of the regression and enter with a large and significant negative coefficient.

Although Bernanke's empirical evidence is probably too limited to prove conclusively that bank failures had a large impact during the Great Depression through their effect on the cost of credit intermediation, the idea and the application to the Great Depression has had a tremendous impact on both macroeconomists and economic historians. Bernanke's common sense discussion of the role of bank failures is quite likely an important source of the voluminous recent theoretical literature on the effect of credit market imperfections. It also probably stimulated a variety of empirical studies of the specialness of bank loans and the role of bank lending in the transmission of monetary shocks to the real economy. In this way, a piece of historical research has had a major impact on the research agenda for the field of macroeconomics.

Bernanke also stimulated a great deal of further historical research. Part of the reason for so much renewed interest in whether the deflation of the 1930s was anticipated or not is related to Bernanke's work. When debts are denominated in current dollars, an unexpected deflation causes widespread defaults and weakens the financial system. Thus, Irving Fisher's notion of the importance of debt deflation is closely related to Bernanke's emphasis on the cost of credit intermediation. Calomiris and Hubbard (1989) make this link explicit. Using a structural vector autoregression, they find that deflations in the late 1800s and early 1900s typically caused financial distress and led to declines in output.

A host of other new books and articles about the Great Depression have appeared in recent years. I like to think that my 1992 article, "What Ended the Great Depression?", tells an important story about the recovery of the United States from the Great Depression. I argue that a huge gold inflow, caused partly by devaluation and partly by political tensions in Europe, caused the U.S. money supply to increase dramatically starting in 1933. I find that this increase in the money supply, rather than fiscal policy or self-correction, explains why the U.S. economy recovered as much as it did in the mid- and late-1930s. In another study of the recovery from the Great Depression, Margo (1988) analyzes the behavior of labor markets using microeconomic data from the 1940 census. One of his most interesting findings is that many workers on public works jobs in the 1930s held these jobs for a number of years. Furthermore, the low wages paid by such jobs may have been quite high relative to the alternatives available to the unskilled workers who got them, especially when the steadiness of public employment is taken into account. These findings may suggest that Darby's (1976) seemingly heretical view that workers on public works jobs should be viewed as employed rather than unemployed may be at least partially correct after all.

THE EVOLUTION OF LABOR MARKETS

Another area in which economics has fused with the rest of economics is labor history. Both labor economists and economic historians have made important strides in the last decade in understanding the evolution of labor markets in the United States. As with the study of the Great Depression, the combination of field specialists and history specialists has led to work that is both theoretically and statistically rigorous and historically sensible and careful.

One topic that has received a great deal of attention is the rise of internal labor markets. In his excellent book, Jacoby (1985) analyzes the change in American labor markets from the spot market, in which the foreman of the factory hired, fired, paid, and harassed workers at will, to the internal labor market, where most aspects of labor relations are governed by accepted rules and procedures and where hiring, firing, and pay are conducted by formal personnel departments. Jacoby argues that, although internal labor markets expanded somewhat during World War I, it really was not until the Great Depression and World War II that there was a substantial rise in the prevalence of personnel departments, which he takes as a prime indicator of the bureaucratization of employment. He attributes the rise of internal labor markets to the labor laws passed during the Great Depression and to the government employment practices that were mandated for firms supplying goods during World War II.

Jacoby's work has stimulated a great deal of subsequent research, much of it critical of his view that internal labor markets emerged rather late in the United States. Carter and Savoca (1990), for example, analyze a variety of survey evidence on job duration in the late 19th century and find that male workers typically stayed in a given job for an extended period. To cite an extreme case, in a survey of workers conducted by the California Bureau of Labor Statistics in San Francisco in 1892, nonunion male workers typically stayed with their current employer for almost 13 years. They argue that this substantial job attachment is inconsistent with Jacoby's view that the U.S. labor market was essentially a spot market until World War I.

Sundstrom (1990) also challenges Jacoby's view that a golden age of flexible wages existed before World War I. Using an annual survey of manufacturing establishments conducted by the Ohio State Bureau of Labor Statistics in the 1800s and early 1900s, Sundstrom finds that during the severe recessions of 1893 and

1908 only a small minority of Ohio workers actually had their wage rates cut. Average earnings did fall, however, because of job sharing and changes in the occupational composition of those employed. Sundstrom suggests that the fact that firms in the late 19th century chose to layoff workers or reassign jobs rather than cut wage rates may indicate that internal labor markets emerged earlier than Jacoby believes.

Both the Carter and Savoca and the Sundstrom studies use detailed survey data from particular states for certain years to analyze the nature of labor markets in the late 19th and early 20th centuries. Although one must be very careful about generalizing from such limited surveys, their use is an important recent development in economic history. The drive to find more hard, microeconomic data on labor markets around the turn of the century holds great promise for finally pinning down the nature of work and pay in America before World War II.

Discovering when employment practices became more structured and formalized is a topic of great interest to macroeconomists precisely because of the issue of wage rigidity addressed by Sundstrom. Wage rigidity is a crucial component of many of the Keynesian and New Keynesian theories of the cause of recessions. If the rise of internal labor markets caused a change in wage flexibility, then this could have led to changes in the nature and severity of business cycles over time. Various studies in recent years have attempted to test whether wages became more rigid at some point in the 20th century. Most recently, Allen (1992) examines whether the cyclical sensitivity of wages was different before World War II and after. He discovers no large change in wage flexibility between 1890 and today. He finds that much of the conventional wisdom that wages used to be more flexible may be an artifact of the crude aggregate wage indexes typically used for the prewar era. If Allen is right, his results could suggest either that the rise of internal labor markets does not affect wage flexibility, or that internal labor markets emerged much earlier than Jacoby believes.

Another historical topic on which much has been accomplished is the changing role of women in the work force. Goldin (1990) presents a comprehensive and compelling portrait of changes in the labor market experience of women, especially married women, between the end of the 19th century and today. She deals with a wide range of topics, including discrimination in women's pay and the decline of bans against the employment of married women in the 1950s. One of Goldin's very interesting findings, and one useful for teaching even introductory students, concerns the relative importance of supply and demand factors in explaining the increase in the labor force participation rate of married women over the last century. Using existing postwar estimates of the determinants of labor force participation, she calculates that prior to 1940, most of the increases in the labor force participation of women were due to supply factors such as increasing education and decreasing fertility. For the period 1940 to 1960, Goldin finds, not surprisingly, that demand forces, such as World War II and the baby boom that increased the demand for school teachers, are the main explanation for the increasing labor force participation of women. Finally, for the period 1960 to 1980, Goldin shows that both demand forces, such as the explosion in clerical work, and supply forces, such as the women's liberation movement, contributed roughly equally to the continuing increase in the fraction of married women entering the labor force.

Another of the many excellent studies in the field of labor history is the work by Raff (1988) on Henry Ford and the $5 day. When Henry Ford in 1914 decided to pay his factory workers $5 a day, his competitors thought he was crazy, and indeed he might have been. Raff, however, tries to see if Ford's behavior can be explained using some of the modern ideas of labor economics. Most important, Raff asks if Ford paid high wages in order to reduce turnover and improve the productivity of his workers; that is, did he pay efficiency wages? This is an important topic because efficiency wage theory has been one of the leading contenders for explaining why firms today seem to pay workers more than the equilibrium wage and why wages do not fall in response to unemployment. If Henry Ford was indeed acting in accordance with this theory, it would make it at least a more plausible explanation for modern experience.

Raff, however, does not find that efficiency was the main motivation for Ford's unorthodox policies. Turnover was not a large problem for Ford because workers could be trained for auto assembly jobs in just a few days. Monitoring worker productivity was also not a problem because, on the assembly line, it was obvious when a worker was not doing his job well or was not keeping up with the pace. Raff concludes that the main reason that Ford paid his workers so much was to prevent unionization and the related threat of sit-down strikes. Because Ford's profits hinged crucially on the efficiency with which his specialized machines were used, worker unrest that prevented the plant from operating would have imposed a huge burden on the firm. Therefore, high wages, which shared the rents from producing Ford's very profitable Model T with workers, turned out to be the profit-maximizing course of action under the tense labor conditions prevalent in the early 1900s.

THE SOURCES OF ECONOMIC GROWTH

A third area where important developments in economic history have occurred in the last decade concerns the sources of economic growth. Perhaps more than in any other area, the melding of economic history with the other subfields of economics has benefitted all of the participants involved in this research program. Economic theorists have learned the facts that need to be explained and economic historians have learned the theories that can motivate new empirical tests and explain perplexing findings.

The theoretical advance that is relevant here is the endogenous growth theory pioneered by Paul Romer (1986, 1987). Romer's insight, which sounds almost ridiculously simple in its crudest form, is that there may be externalities from capital formation. When a society invests in capital, output may increase by more than the direct result of having more machines. It may rise because technological change is more rapid when there are a lot of machines around and workers think of ways to improve them. It may also rise because there is learning by doing or knowledge spillovers from one industry to another. What Romer shows is that this simple insight has enormous implications for how one thinks about the process of economic growth. For example, in his model, an increase in capital formation can explain a permanent increase in the rate of growth of output, not just a temporary acceleration in the traverse between steady states.

Another theoretical advance closely related to that of Romer is work by Murphy, Shleifer, and Vishny (1989) on the role of income distribution in economic growth. They show in a simple model that the presence of a large middle class may foster industrialization and growth by generating demand for the kinds of goods amenable to mass production and for which spillovers may be substantial.

The idea that learning by doing could be important and that there could be externalities from capital formation is very appealing, and I might add, not very surprising, to economic historians. Indeed, as a justification for his models, Romer cites the work of Schmookler (1966) on the positive correlation between patenting activity and investment. He also could have cited the classic study by David (1970) on learning by doing in the American cotton textile industry. The idea that income distribution could also matter is derived very closely from the work of Rosenberg (1972), who shows that the presence of a large group of yeoman farmers in the United States increased the demand for goods such as basic guns, and thus stimulated a high growth, high productivity sector of early American industry.

In recent years, work by economic historians has tended to confirm and flesh out the new growth theory. One of the most important contributions is a short article by De Long (1988) that challenges Baumol's (1986) finding that the level of per capita income of various countries has converged over time. De Long shows that Baumol's results are almost entirely due to sample selection problems: by looking only at countries that were rich in 1979, Baumol biased his results toward finding convergence. De Long shows that if one looks instead at countries that were rich in 1870, one finds that levels of per capita income have not converged over time. Countries such as Argentina and Spain that looked poised for success in 1870 have not done as well as other countries, such as the United States and Germany, that were also prosperous in 1870.

De Long's results are relevant to Romer's theory because they may reveal something about the nature of technological change. If technological progress were exogenous in the way the Solow model seems to imply, then it should be the case that as knowledge spreads from country to country, per capita incomes converge. On the other hand, if technological progress is mainly endogenous, one might expect the rich to get richer; countries that invest more get more learning by doing and grow even faster. Because De Long finds no evidence of convergence, his results are at least partial corroboration of Romer's conjectures.

Wright's (1986) excellent book, *Old South, New South,* is also relevant to the debate about the process of economic growth. Although Wright covers many topics in this wide-ranging book, one of the most interesting is the question of why the American South was slow to industrialize during the 19th century. Wright's case studies of particular industries suggest that the late start to southern industrialization was a major factor inhibiting faster growth and development. By starting late, the South lost out on developing a trained industrial labor force. As a result, when industrialization began after the Civil War, labor productivity was low and the incentive to set up factories was less than in the North. Wright also shows that the lack of an indigenous technological community meant that the innovation necessary to adapt northern technologies to the particular conditions and natural resources of the South were missing. Both of these stories are very much in the spirit of Romer's analysis that there are externalities from capital formation, or conversely, that the absence of capital formation has very large adverse consequences.

Although the works of De Long and Wright have been supportive of the Romer view of endogenous growth, clearly much more work is needed in this area. Indeed, if I were the social planner, I would direct much more historical research to understanding the process of economic growth and technological change. I think much could be gained by looking more at differentials in growth across regions or countries and at the spillovers from concentrating production in certain places or at certain times.

One piece of careful and interesting research that does exactly this kind of analysis is an article by Clark (1987) who tries to understand why textile workers in countries such as India and China in the late 19th and early 20th centuries were so much less efficient than textile workers in the United Kingdom or the United States. Clark's conclusion is that none of the conventional explanations can explain the productivity differential. He argues that the capital was the same, the managers were the same, the raw materials were the same, the worker training was the same; everything identifiable was too similar between, say, India and England to explain a nearly six-fold difference in productivity. He, therefore, concludes that what matters is local culture; in some areas, workers work harder than in others. One piece of evidence in favor of this unconventional view is that workers who migrated from less-productive countries to more-productive countries became as productive as native workers.

If Clark is right, and his work has generated a heated-enough debate that it is questionable, he may present an important challenge to Romer. Maybe technological change and growth are not the endogenous result of capital formation. Instead, perhaps local attitudes, religion, and inexplicable cultural factors are the real determinants of productivity and progress. Only extensive research of other countries and other industries will ever resolve this important debate.

THE ROLE OF FINANCIAL INSTITUTIONS

A fourth area of research in economic history in which substantial progress has been made in the last decade concerns the role of financial institutions. This is a topic that is obviously closely related to the sources of economic growth just mentioned: if capital formation is very important to economic development, then financial institutions that affect capital formation are also very important. This is again a topic on which economic historians, macroeconomists, and financial economists have all contributed greatly to the literature.

One of the most interesting articles in this area is a study of New England banks in the early 1800s. Lamoreaux (1986) shows that New England banks in the early national period were very peculiar institutions. For the most part, they were extensions of the kinship groups that had dominated the New England shipping industry in the colonial period. As capital requirements increased with the rise of industry, the kinship groups opened banks and solicited deposits from outsiders. However, most of the loans were then plowed back into the family business. Lamoreaux points out that despite their idiosyncracies, the New England banks functioned fairly well. The banks were highly capitalized; indeed, they were almost more like investment pools than genuine banks. The high capitalization meant that deposits were very safe and this presumably encouraged savings through intermediaries. The banks also appear to have made more loans to businesses than was previously thought. By going through the loan records of various banks, Lamoreaux is able to show that what appear to be short-term loans to individuals, were often loans to the owners of firms that were rolled over many times.

Lamoreaux's portrait of early New England banks in some ways challenges the conventional view that advanced financial institutions are crucial for investment. Or, at least, it challenges the conventional view of what constitutes advanced financial institutions. The fact that these seemingly peculiar, nepotistic, investment pools mobilized the funds necessary to fund the early industry of New England suggests that economies may find ways to adapt when ideal institutions are absent.

An article by De Long (1991) makes a similar point in a different context. De Long, like Lamoreaux, asks whether a peculiar, imperfect financial institution had a positive benefit for the firms associated with it. Morgan and Company was the first large investment bank in the United States. During the early decades of the 20th century, many reformers thought that Morgan and Company, which held a virtual monopoly on certifying stock issues and arranging mergers, imposed an unfair and destructive tax on firms by charging high fees for its services. Others feared that Morgan could swindle investors by falsely certifying weak firms.

De Long constructs a unique data set that includes stock prices, dividends, capital stock, and other variables for the firms closely associated with Morgan and Company and for a control group that had nothing to do with the investment syndicate. The control group is chosen to match the size and industrial composition of the Morgan firms. De Long then examines, among other things, whether stock prices were higher relative to dividends for Morgan firms than for non-Morgan firms. He finds that J. P. Morgan's men did indeed add value; that is, stock prices for firms that had a J. P. Morgan partner on their board of directors were consistently higher relative to measured fundamentals than those of firms with no relation to Morgan. De Long then tries to figure out just what the Morgan connection actually provided. He concludes that the Morgan relationship did not just enable firms to gain monopoly power, but rather provided valuable guidance in the choice of top-level management.

Ramierez (1992) provides another interpretation of De Long's findings. Using additional information on liquid assets for the same sample of firms, he argues that what the J. P. Morgan connection provided was cash flow. He finds that Morgan-associated firms were able to invest when conditions were right, whereas the investment expenditures of non-Morgan firms were very sensitive to current cash flow. Whether Ramierez or De Long is right about what Morgan provided, both suggest that what was conventionally thought to be a noncompetitive, highly questionable financial institution actually provided a very useful service to early industrialists. In this way, they echo Lamoreaux's view that even financial institutions that are not advanced or perfect in the conventional economic sense, may foster development.

Two papers by Barsky and De Long (1990, 1992) sound a related theme about the American stock market in the 20th century. Ever since Shiller's (1981) article on the excess volatility of stock prices, economists have been puzzled by the fact that stock prices seem to move much more than would be justified by changes in dividends or other fundamentals. This fact has naturally led to conclusions about the inefficiency or the imperfection of the U.S. stock market, with all of the related implications for distortions in the cost of external finance.

Barsky and De Long, however, show that much of the seeming excess volatility of stock prices between 1900 and today can be explained by understandable swings in expectations. As an empirical matter, Barsky and De Long argue that if the future growth rates of dividends are highly uncertain, investors are likely to put more weight on recent dividend performance than on dividend growth in the distant past. They show that for plausible specifications, such rules of thumb could give rise to the long swings in stock prices observed in U.S. data. The authors then go on to look at qualitative evidence of investors' expectations. They find that professional stock analysts seemed to interpret fundamentals in exactly the way predicted by their rule-of-thumb equations. Thus, the long swings in stock prices that seem to be unjustified by actual performance, can nevertheless be explained by long swings in expectations. The obvious implication of this finding is that the U.S. stock market was not a highly imperfect financial institution, but rather a well-functioning market that reflected the expectations of the participants.

Although the Lamoreaux, De Long, and Barsky and De Long articles contribute much to our understanding of particular institutions, the role of financial institutions in economic development is one that I think still requires much more research. What these authors have shown is that particular American institutions that were conventionally thought to have hampered investment really were not so bad. We still do not know how much more capital formation would have occurred if more such institutions or better institutions were available here or in other countries. We also do not know if more advanced financial institutions would have caused the pattern of industrialization to be different than it actually was. For example, one of the most intriguing questions that has not yet been adequately resolved is that raised by Davis (1966) on whether the difficulties in raising funds in the late 19th century in the United States was an important source of the increasing concentration of American business. Although Davis presents some very interesting case studies, broad empirical analysis of this relationship has yet to be done.

One article that stresses the failures of American financial markets rather than the strengths is Calomiris and Hubbard (1992) on the Undistributed Profits Tax of 1936–1937. Calomiris and Hubbard point out that the Undistributed Profits Tax, which imposed an extra tax on firms that held extensive retained earnings, provides an excellent experiment for comparing the costs of internal and external finance in the interwar period in the United States. Firms that would chose to hold on to retained earnings and pay the substantial additional tax rather than distribute the earnings as dividends, must feel that the cost of obtaining investment funds through issuing stocks or borrowing from banks was even higher than the tax on retained earnings. One would expect that the investment decisions of firms with such high costs of external finance would be very sensitive to cash flow.

Calomiris and Hubbard's finding is that a substantial number of firms did choose to pay the Undistributed Profits Tax, indicating that even a fair number of large, publicly traded firms faced a very big wedge between the costs of internal and external finance. Moreover, the investment expenditures of these firms with a high wedge between internal and external financing costs were much more sensitive to cash flow than those of firms facing less of a differential. In addition to showing that American capital formation in the prewar era in general may have been inhibited by imperfections in the financial system, the Calomiris and Hubbard story may provide information on why the recovery from the Great Depression was not faster. If liquid assets were an important determinant of investment, then a prolonged depression that wiped out liquid assets would sow the seeds of its own slow recovery.

THE STABILIZATION OF THE POSTWAR ECONOMY

A fifth area of economic history that has experienced a flurry of recent activity involves possible changes in the nature and severity of business cycles over time. This is another example that fits well my theme of the blurring of the lines between economic history and the rest of economics. The question of whether business cycles have gotten shorter or less severe over time is one that has been analyzed by both macroeconomists and economic historians and one whose answer is important for further study in both areas.

The pioneering work on changes in business cycles over time was conducted by Wesley Mitchell (1927; and Burns and Mitchell 1946). Mitchell illustrates beautifully the view that economic history was once not seen as a separate subfield of economics, but rather an integral part of all subfields. Mitchell probably would have defined himself as a business cycle economist. Yet, his work was explicitly historical. To define what a business cycle was, he used the pattern of cyclical behavior shown in the second half of the 1800s and the early part of the 1900s. To construct a theory of the cause of cycles, he looked at historical evidence on what caused previous recessions. To predict what would happen in the future, he looked at what had happened in the past. History, theory, and statistics all blurred together in Mitchell's work.

In more recent decades, changes in cyclical behavior have typically been the purview of macroeconomists. Several studies point out that almost all indicators of real and nominal economic activity are dramatically more stable after World War II than in the late 1800s and early 1900s. The authors of these studies explain the phenomenon of the stabilization of the postwar economy in a variety of ways. Tobin (1980) draws the obvious conclusion that since stabilization policy and stabilization occurred at roughly the same time, Keynesian policy should be given at least some of the credit. Baily (1978) offers a more subtle argument: the sheer availability of stabilization policy caused firms to react less to shocks, and thus policy stabilized the economy without having to be used. De Long and Summers (1986) offer the novel hypothesis that the greater price flexibility of the prewar era was destabilizing because it led to extreme swings in real interest rates. Thus, the real stabilization was a consequence of the nominal stabilization.

In a series of articles (1986a, 1986b, 1989), I used the historian's healthy skepticism for accepted facts and questionable data to challenge not just the interpretations of the stabilization of the postwar economy, but the occurrence of the phenomenon itself. I show that the apparent stabilization of the American economy after World War II is to a large degree an artifact of changes in data collection procedures. The starting point of this work was the realization that a series pulled out of *Historical Statistics* is often not one consistent series measured in the same way over time, but, rather, different series spliced on to one another. Indeed, in the case of most aggregate macroeconomic series, such as gross national product (GNP), the unemployment rate, and industrial production, major changes occurred in the data collection procedures around World War II. The series as we think of them today only started being collected using sophisticated procedures in the 1940s. The estimates for the decades before 1940 were all constructed long after the fact using whatever bits and pieces of data were found in census records, firm archives, and special studies.

My reading of the methods used to convert such bits and pieces of data into aggregate estimates led me to wonder if these changes in procedures could account for the apparent stabilization of the postwar economy. For example, to estimate unemployment before 1930, Lebergott (1964) first estimated the labor force and then subtracted an estimate of employment. To estimate employment in certain key industries, he assumed that employment moved one-for-one with output. However, for the postwar period, we know that the labor productivity is strongly procyclical: rather than moving one-for-one with output, employment falls less than output in recessions and rises less than output in booms. If the same were true in the prewar era, Lebergott's measures, which do not take this into account, would show employment falling more than was probably true in recessions and rising more than was probably true in booms. This would make prewar unemployment, which is measured as a residual, excessively volatile. The methods used to construct two other series, real GNP and industrial production, also led me to wonder if they might be excessively volatile in the prewar era as well.

To see if such changes in data collection and construction techniques really are large enough to account for the apparent stabilization of the postwar economy, I conduct the following exercise. I start by admitting defeat; there is simply no way to go back and conduct the same kind of surveys that are used today to measure unemployment or GNP. What I can do is throw away the modern data and measure modern unemployment or other series using the same bits and pieces of data and the same assumptions that were used to construct the historical series. The result of this exercise is the creation of series that are consistent over time; consistently bad, but consistent nonetheless.

These consistent series for the unemployment rate, real GNP, and industrial production show much less change in the severity of cycles over time. Indeed, recessions in the period before World War I or before 1929 look remarkably similar to recessions after 1947. The Great Depression, however, most definitely does not disappear. In some ways it stands out even more than in the conventional macroeconomic indicators. Rather than just being the worst of many bad prewar recessions, it looks like the anomalous collapse of an economy that is very similar in the 40 years before and after.

The fact that the prewar extensions of three major macroeconomic indicators were all constructed in a way that accentuated their volatility is not as surprising as it may seem. At the time that all of these series were being created, the accepted technique was to take whatever bits of information were available and assume that the aggregate that one was trying to measure moved one-for-one with the pieces of available data. Because a certain amount of cancelling out of fluctuations typically occurs when many series are added together, interpolating by a limited number of series will tend to accentuate volatility. More important, the bits of information available for the prewar era often tended to be particularly volatile ones. In which states is unemployment most likely to be measured? Those that have an unemployment problem. What commodities are likely to be measured? Those that are easy to count such as pig iron and raw cotton. Such industrial materials are also goods that tend to be quite volatile because of inventory fluctuations. Because these limitations in technique and the type of data available are likely to apply to most macroeconomic indicators, it is not surprising that the series I examine all show similar inconsistencies over time. Nor would it be surprising if series that have not yet been tested also have similar problems; indeed, it would be more surprising if they did not.

Those who are familiar with this literature will know that my results have not gone unchallenged. Balke and Gordon (1989), for example, create a new GNP series for the pre-1929 era that is just as volatile as the Kuznets series (published and analyzed in Kendrick 1963) it replaces. Weir (1986) suggests that my technique of carrying the old methods forward in time may overstate the similarity of the size of prewar and postwar recessions because of structural changes. It is possible that the assumptions used to create the old series were correct for their time period, but are not correct for today.

However, my work has also been confirmed by other studies. For example, Shapiro (1988) examines changes in the volatility of stock prices over time and argues that a relationship should exist between the real economy and the stock market. Because he finds that stock prices have not become more stable over time, he concludes that my findings on the absence of stabilization of the real economy are plausible. Sheffrin (1988) looks at the data for many European countries that started to keep official output statistics long before the United States. He finds that no country except Sweden has become noticeably more stable between the pre-World War I and the post-World War II eras. If one believes that the experience of major industrial countries should have been similar over time, then Sheffrin's results also lend credence to my findings.

However the debate about the stabilization of the postwar economy is eventually resolved (and I naturally hope that I am eventually judged to have been at least partly right), I think many positive results have already emerged. Most obviously, researchers have become much more cautious about their data. Macroeconomists seem much less likely to include prewar series in their regressions without first checking how the series were constructed. Even more encouraging is the fact that many new data-collection projects seem to be under way. Susan Carter, Roger Ransom, and Richard Sutch, for example, are attempting to create new estimates of unemployment using the survey data available for various states in the late 1800s and early 1900s. Jeffrey Miron has been overseeing a project at the National Bureau of Economic Research to make available the many disaggregate series that underlie the work of Simon Kuznets, Solomon Fabricant, and other creators of early macroeconomic indicators. Such projects hold great promise for increasing our factual knowledge about the prewar macroeconomy. Finally, the debate about stabilization may have stimulated economists to question just what factors are likely to have affected the cycle over time. Once the facts come into doubt, theories about what should have happened, and about the role of such factors as monetary policy, supply shocks, and credit market imperfections seem to have multiplied.

Perhaps more than any particular finding or direct implication, the fact that the debate about stabilization and the new data collection efforts are being carried out by a mixture of economic historians and macroeconomists is the most desirable development of all. As with all of the other recent developments in economic history that have been discussed here, the bringing together of researchers with different perspectives has not only stimulated exciting research, it has also meant that the lessons of history have been incorporated into other fields. In this way, the end of economic history has really been just the beginning of better and richer economics.

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